

nature. But when an explosion subsequently occurred in the mine, and traversed the whole length and breadth of the workings, which were known to be practically free from fire-damp at the time, producing the most disastrous effects, the fallacy of the conclusions drawn from the experimental results was rendered abundantly evident.

In the whole of my papers on this subject, and most pointedly in my last article on coal-dust explosions published in *Iron*, in the year 1878, I have carefully indicated that a difference was to be expected in the behaviour of dust ignited under the two conditions named. It has therefore been with feelings of considerable surprise that I have observed members of the French, English, and German Mining Commissions, and others who have investigated this subject since the publication of my first paper, one after the other pronounce some very decided opinions as to the relatively subordinate part which coal-dust plays in a colliery explosion, while at the same time they were neglecting to take into account this very simple and yet all-important element. W. GALLOWAY

#### VESUVIAN ERUPTION OF FEBRUARY 4, 1886

THE rent that was formed on May 2, 1885, in the upper part of the great cone (*NATURE*, vol. xxxii. p. 55) gave issue to lava until December 25. A small quantity again issued between January 2 and January 5, 1886, after which no more made its appearance till this new outburst. In consequence of the rise of level of the magma in the chimney, the cone of eruption has grown very much during the last month.

On February 4, at about 8 p.m., lava broke forth at the foot of the old crater ring of 1881-2 at a point bearing from the main vent about  $10^{\circ}$  W. of N., traversed the crater plain, which is here very narrow, in a somewhat oblique direction, and ran down the slope of the cone between north and north-north-east. The lava soon reached the foot of the cone, but even up till midday to-day, when I left the mountain, it had not yet commenced to cross the Atrio del Cavallo. The eruption took place from probably the same dyke that gave rise to that of January 9, 1884.

To-day, February 6, the lava bubbles up like a spring at the foot of a hill, and flows for some distance in a kind of trough which it has raised on each side of itself above the level of the crater plains. After a short distance it enters one of its own tunnels to reappear again at some distance. It was very interesting to watch its welling, and from time to time the bursting of a steam bubble as big as a bucket, which would throw up splashes against the imperfect arch at the immediate exit. These splashes partly adhered to the roof and partly fell, drawing out the suspended portion into irregular strings, illustrating the formation of the stalactitic lava that is so common in lava fumaroles.

The chloride crusts in the neighbourhood were uncommonly rich in copper, so that my boot-nails were thickly plated with that metal.

The lava makes its appearance at about 100 yards from its entrance in the tunnel near the main spring, although it is now divided into two streams. The eastern, which is the largest, is 1 metre broad; I plunged a stick in to the depth of 1 metre, but the shortness of the stick and the great heat prevented me from touching the bottom. The current ran at the rate of 1 metre per 6 seconds, which, making allowance for viscous drag at the edges and bottom, will give an output of at least 360 cubic metres per hour, or at least 17,280 cubic metres during the 48 hours up to 8 o'clock this evening. The more western stream was 50 centimetres broad, over 1.20 metres deep (as far as I could reach with my stick), and flowed at the rate of 1 metre in 8 seconds. Giving a loss of 2 seconds of speed from drag at sides and bottom, we have an output, for 48 hours, of 10,368 cubic metres.

The two streams together would, therefore, have afforded, since the commencement of the eruption, 27,648 cubic metres.

As the altitude of the lateral outlet is much more than that of May 2, both on the night of the eruption and the following one, the volcano showed the *first stage of activity* as judged by the appearance of the main vent.

This winter the mountain has been covered by snow several times, and to-day it extends down nearly to the level of the Observatory. During our ascent we had to walk through a stratum of about 8 inches, though much thicker in the drifts. Two-thirds of the crater and part of the cone of eruption were also covered.

I should have sent news yesterday, but, on attempting an ascent, I was driven back by wind, rain, and mist.

Naples, February 6

H. J. JOHNSTON-LAVIS

#### TIDAL FRICTION AND THE EVOLUTION OF A SATELLITE

A PAPER by Mr. James Nolan has recently appeared which is devoted to an adverse criticism of my views concerning the effects of tidal friction as a factor in the evolution of the moon and earth.

The title of the pamphlet, "Darwin's Theory of the Genesis of the Moon,"<sup>1</sup> shows, I think, that the author has misconceived the scope of my work. It was not supposed that the investigation threw light on the actual mode of genesis of the moon, but was rather applicable to the subsequent history of the moon and earth. Mr. Nolan attributes to me views as to the condition of the moon immediately after her birth which do not appear a just interpretation of my writings, and although it might have been well to use more guarded expressions in some passages, the justice of his condemnation of the whole theory cannot be admitted. He sums up his case by the three following propositions:—

"(1) That the moon could not have existed bodily so near the earth as the greatest initial distance fixed.

"(2) That in any form possible there she could not have receded by the agency assigned—tidal friction.

"(3) That, if a modification be made by allowing her to have separated at a greater radius than that corresponding to a period between 2 and 4 hours, the moon would be no longer traceable to the earth's *present* surface on which condition the theory has been founded."

The first of these propositions is interesting, and I have to thank him for drawing my attention to it.

When a small satellite revolves about a planet with a certain proximity, the sum of the centrifugal and tidal forces may be such as to overbalance the gravitation towards the centre of the satellite. When this is the case, the satellite cannot exist as a single mass. The complete solution of the problem, concerning which Mr. Nolan adduces certain elementary considerations, is of extreme difficulty. At present I do not wish to go into this question, but shall consider the point on another occasion. It may, however, be admitted that the moon could not subsist as a single continuous body with its surface in contact with the earth.

On p. 4 he quotes a passage from the abstract of one of the papers (*Proc. R.S.*, No. 200, 1879), which must be surrendered; it is as follows:—

"The coincidence is noted in the paper that the shortest period of revolution of a fluid mass of the same mean density as the earth, which is consistent with an ellipsoidal form of equilibrium is 2 hours and 24 minutes; and if the moon were to revolve about the earth with this periodic time, the surfaces of the two bodies would be almost in contact with one another."

Now, since 1879 it has been shown by Sir William Thomson that the ellipsoidal form referred to could not

<sup>1</sup> Geo. Robertson and Co., Melbourne, Sydney, Adelaide, and Brisbane, 1885. Pp. 16.

subsist, because it is dynamically unstable. It does not, then, seem worth while to consider the remarks made on that passage.

With regard to the first proposition that, if the moon separated from the earth near the present earth's surface, it can only have subsisted as a flock of meteorites, my own words may be quoted as follows :—

"The planet then separates into two masses, the larger being the earth and the smaller the moon. I do not attempt to define the mode of separation, or to say whether the moon was initially more or less annular. At any rate it must be assumed that the smaller mass became more or less conglomerated, and finally fused into a spheroid, perhaps in consequence of impacts between its constituent meteorites, which were once parts of the primæval planet. Up to this point the history is largely speculative, for, although the limiting ellipticity of form of a rotating mass of fluid is known, yet the conditions of its stability, and *a fortiori* of its rupture, have not as yet been investigated. . . . At some early stage in the history of the system the moon has conglomerated into a spheroidal form."<sup>1</sup>

When, however, Mr. Nolan goes on to his second proposition, and states that this amounts to saying that the moon must have been a ring of fragments revolving in the plane of the equator, and that such a ring must be uniformly distributed and therefore incompetent to raise frictional tides, it is not easy to follow him. Is there any objection to the existence of a flock of meteorites? And would not such a flock raise tides in the planet which, if subject to friction, would introduce forces tending to make the meteorites recede? It seems that there is no such objection, and that the flock of meteorites would follow the same fate as the satellite when conglomerated in a single mass.

The difficulties which are raised by the author in the conception of the conglomeration are such as meet us in all evolutionary theories, and whether or not it is possible as yet to see our way mentally through the changes which may have taken place, yet it is generally admitted that conglomeration took place in some way.

He then points out that no other satellite is traceable up to the surface of its planet, and concludes that it is a coincidence that the masses and periods of the moon and earth are apparently such as fit into the theory. No one has pointed out the non-existence of such a satellite more clearly than I,<sup>2</sup> but the absence of reference to my work seems to show that Mr. Nolan has not looked at it. It is not then surprising to read: "Is it not very illogical to suppose that the moon originated in a way which cannot have been the way of origin for other planets and satellites?" And the reader of this sentence would hardly think that my position is that there is a probability that a cause which was subordinate in the history of the other planets was predominant in the case of the moon and earth, and that it is proved numerically that in the terrestrial sub-system the actual distribution of masses and momenta (the factors governing tidal friction) differs at least as much from the corresponding factors in the other planetary sub-systems as the supposed modes of evolution.

On p. 13 we read :—

"There is a law, according to which two heavenly bodies cannot revolve about their centre of gravity with their surfaces nearly in contact, unless one be smaller than the other by a certain amount, and, further, that the small one be denser than its companion by a certain value."

I do not know where to find the proof of such a law, and at the present moment am disposed to doubt its correctness.

<sup>1</sup> *Phil. Trans.*, part 2, 1880, pp. 880-81.

<sup>2</sup> "On the Tidal Friction of a Planet attended by several Satellites" *Phil. Trans.*, part 2, 1881.

Next, on p. 14, we find :—

"Rapid rotation would never cause a quantity of the matter of a body to become piled up at one particular place, and form into a separate single body there of any appreciable size."

Now very recently M. Poincaré has rigorously proved in a very remarkable paper<sup>1</sup> the possibility and even the dynamical stability of such a "piling up," and has given a sketch of the mode of separation of a portion of the mass of rotating fluid. In a paper of my own, now nearly finished, the same problem is treated, but from a different point of view.

It will be perceived from the quotations that the pamphlet is true to its title, and refers almost entirely to the genesis of the moon. This affords some proof that my speculative remarks hazarded as to the mode of origin of the moon, were not so guarded as was intended. The justice of the third of Mr. Nolan's propositions may, however, be denied, and certainly the theory cannot be held to depend on the genesis of the moon at the *present* surface of the earth.

The present opportunity will be convenient for a short reiteration of my point of view with regard to the whole subject.

In tidal friction we have a *vera causa* of modifications in the configuration of the earth and moon. If we adopt provisionally the hypothesis of an adequate lapse of time, we can trace the changes, and find that the obliquity of the ecliptic, the eccentricity of the lunar orbit, and its inclination to the ecliptic (all unmentioned by Mr. Nolan), the lunar periodic time, and that of the earth's rotation, are co-ordinated together by supposing that the moon first had a separate existence at no great distance from the present surface of the earth, and with small differential motion with respect thereto. Then it is maintained that this co-ordination is so remarkable as to give good reason for accepting the hypothesis as in accordance with truth. Concerning the earlier stage in which the moon may be supposed to have separated from the earth, nothing more than conjecture is possible, but undoubtedly the condition adduced by Mr. Nolan escaped my notice.

In examining the rest of the solar system, it is found that, amongst other things, the Martian satellites afford a striking confirmation of the influence of tidal friction, and that the system of the moon and earth presents features so distinct from those of the other planets, as to justify the belief that tidal friction, subordinate in its influence on the other systems, may have been predominant in our own. The theory is also found to throw light on the distribution of satellites in the solar system.

It is as yet too soon to say how far these views embody the truth, but even should they be found untenable, yet certainly the determination of the effects of tidal friction on a system of planets and satellites is a problem of physical astronomy which was well worthy of attack.

G. H. DARWIN

#### ON THE SOUND-PRODUCING APPARATUS OF THE CICADAS

HAPPENING to refer to Prof. Jeffrey Bell's "Comparative Anatomy and Physiology" on the question of the sounds produced by insects, I read, with reference to the Cicadas :—"The sound seems to be produced by the vibration of membranes, placed on either side of the stigmata of the metathorax, and set in motion by the respiratory air" (p. 389).

As this wind-instrument theory of Landois seems to be supplanting, in our text-books and popular natural histories (*e.g.* Cassell's), the drum theory advocated by Réaumur and the earlier writers, I think it permissible to draw attention to certain observations I made on this

<sup>1</sup> *Acta Mathematica* (1885), 7, 3, 4, "Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation."